has returned it to me as *M. minimus* var. *marginicollis*, Schil. My specimen is a ♂. I append herewith a translation of Schilsky's original description of this form: "The black colour in some examples from Herzegovina (von Hopffgarten), and not rare there, extends so that only the borders of the thorax remain yellow; in some examples only the hind corners are yellow, whilst the arched side portions of the thorax always remain yellow. The tibiæ are then also darker, and the first joint of the antennæ alone remains yellow. In this form, confusion with *pellucidus* can easily take place" (Schilsky, *Deut. Ent. Zeit.*, 1892, p. 198).

Variation in Lepidoptera—A Criticism.

By J. W. TUTT, F.E.S.

We have before us the report of the Lancashire and Cheshire Entomological Society, for February 17th, in which it is stated that Mr. W. Mansbridge read a paper entitled "Variation in Lepidoptera," in which he "enumerated the different classes of variation as generally understood by lepidopterists, and referred especially to a phase of variation which has not evoked the amount of interest its importance warrants, *viz.*, colour changes from yellow or ochreous to red or brown, and modifications of these. The author considered these variations as proceeding upon parallel lines to melanism, and probably arising in a similar way, (1) by variation from a commonly occurring form in the Darwinian sense, and (2) by mutation or sudden leaps in the sense enunciated by De Vries." We had hoped that we could have passed this report over as a poor or inaccurate summary by the secretary, but as Mr. Mansbridge is secretary, and signs the report, one can only assume the report is his own.

We do not notice that Mr. Mansbridge makes any further reference of importance to the particular phase of variation that he specially mentions, *viz.*, from yellow or ochreous to red or brown, and modifications of these, nor does he give any details referring to special species in which these changes occur, nor discuss the changes from an experimental point of view. Probably there were such in his extended paper, but this being so, a mere statement, in his press report, that "this form of variation has not evoked the amount of interest that its importance warrants," appears to serve no useful purpose, nor does Mr. Mansbridge's report suggest that he is aware that considerable attention has been devoted to this phase of the subject. It is now some sixteen years since we wrote, as an introduction to *The British Noctuae and their Varieties*, vol. ii., a comprehensive chapter on "The Nature of Insect Colours, and their Genetic Sequence," occupying no fewer than 16 closely printed demy 8vo. pages, of which more than two-thirds are devoted to the particular phase which, Mr. Mansbridge suggests, has not received attention. Our thesis on the genetic sequence of insect colours, among other things, dealt with two presumably progressive forms of development, *viz.*, (1) through white, yellow, orange, red, brown and black, (2) through white, yellow, green, red (or brown), purple and black.

The subject is then considered in detail, illustrated entirely by species that occur in Great Britain, and by facts that have been accumulated by British lepidopterists. Probably Mr. Mansbridge

considers the facts erroneous, but if so they should be so proved; perhaps he thinks the arguments unsound, then he should disprove them; at any rate the subject which Mr. Mansbridge suggests has not received attention is discussed at length and supported by facts which Mr. Mansbridge possibly could controvert. Yet we wonder whether Mr. Mansbridge has ever read or studied the details.

This, however, may have been an oversight of Mr. Mansbridge's, but has he also overlooked the paper on "Pupal development and Colour of Imago" (*Ent. Rec.*, iv., pp. 313-315) and the long series of papers on "The Nature of Insect Colours" (*Ent. Rec.*, vi.)? In this series, Dr. Riding, Dr. Freer, Rev. C. R. N. Burrows, and others, distinctly proved the existence of the "pigment-factor," which gives us a basis for variation. Then there are "Changes in the Colour of the pupa of *Epinephele ianira* just previous to emergence" (*Ent. Rec.*, viii.). "Development of the wing, wing-scales, and their pigments in Butterflies and Moths" (*Ent. Rec.*, ix.). "On the wing-scales and their pigment in Lepidoptera" (vol. ix.), "Aberrations of *Abraxas sylvata* (*ulmata*)" (*Ent. Rec.*, ix.). "The variation of *Hemerophila abruptaria*" (*Ent. Rec.*, x.), and very many other similar papers. One wonders, too, whether Mr. Mansbridge has missed Dr. Mayer's able work. If Mr. Mansbridge has missed all this, is he justified in saying that this particular phase of variation has not evoked the amount of interest its importance warrants? If he has studied these, then we would ask whether, in view of the great importance of the subject, Mr. Mansbridge has in any way attempted to fill the lacunæ that those of us who have done at least something know to exist?

When we come to the statements of Mr. Mansbridge's paper, one wonders what he means. He states that he considers that "the variations from yellow or ochreous to red or brown and modifications of these proceed upon parallel lines to melanism, and probably arise in a similar way (1) by variation from a commonly-occurring form in the Darwinian sense, (2) by mutations or sudden leaps in the sense enunciated by De Vries."

On these points we should like to ask a question—In what way does melanism arise? Mr. Mansbridge gives the answer. (1) "From a commonly-occurring form in the Darwinian sense." But does not this beg the whole question? What we want to know is—what gives rise to the commonly occurring form that becomes melanic? When we know this we shall begin to know where we are. (2) "By mutations or sudden leaps in the sense enunciated by De Vries." We should like to have some credible evidence on "sudden leaps" that have achieved the ultimate end of producing "melanic" races, "brown" races, or "red" races, from yellow or ochreous specimens arising *per saltum* as aberrations.

Mr. Mansbridge duly sets "aside the first as more or less affecting all species," and then it is stated that "he showed how, practically, all definite melanic forms, falling in the second class, of which we have records, have, when first noticed, been of very local occurrence, as the majority still are, a few only having spread, in comparatively recent times, over large areas, and he noted, when this has been the case, that the particular species, *e.g.*, *Tephrosia bimdularia* var. *delamerensis*, *Amphidasys betularia* var. *doubledayaria*, *Hybernia marginaria* var. *fuscata* and *Diurnea fagella* black var., are common, and generally

distributed so that transported specimens could easily continue their race wherever they might be carried."

One learns from this not over-clear statement, that the melanic forms noted by Mr. Mansbridge, fall in his second class, *i.e.*, that the *T.* var. *delamerensis*, *A.* var. *doubledayaria*, *H.* var. *fuscata*, and *D. fagella* black var. have arisen by "mutations or sudden leaps." We should like to know whether Mr. Mansbridge has any evidence that any of these varieties originated by sudden leaps, and whether he thinks the statement "that all definite melanic forms . . . have been, of local occurrence, when first noticed," is satisfactory or sufficient evidence of the assumed facts? We wonder whether Mr. Mansbridge would be surprised if lepidopterists wanted more definite data before accepting such a sweeping generalisation that these varieties originated by "mutation," as understood by Mr. Mansbridge. We are under the impression that we first collected together the earliest notices that appeared on the variation of these melanic forms in our work *Melanism and Melanochroism in British Lepidoptera*, pp. 8 *et seq.* (particularly pp. 12-17), and, as we are not aware of any earlier record than these, we assume that Mr. Mansbridge has based this generalisation on the facts as there stated, or the same obtained elsewhere. If there are other facts known to Mr. Mansbridge and not to us, our opinion might, of course, be considerably modified, but, if not, then Mr. Mansbridge appears to us to be making a marvellous assertion on very slender and treacherous grounds.

Again, if these forms have all increased by sudden leaps ("mutations" is evidently the correct thing!), how does Mr. Mansbridge explain the intermediate forms that we have seen in abundance of both sexes of *Tephrosia crepuscularia* (Mr. Mansbridge's *T. biundularia*), *Hybernia marginaria*, and *Diurnea fagella*. Does Mr. Mansbridge really think the specimens of these species are either ochreous or black? If so, he cannot know much of the species? If not, well, then, where does the Kangaroo habit come in?

Also, if these melanic species, as Mr. Mansbridge asserts, have been formed by leaps ("mutations"), how does he explain the necessity of "transported specimens" to "easily continue their race wherever they might be carried"? Surely "black specimens" can leap into existence as well at one place as another, and, this being granted, the carrying is superfluous. We may note, too, that "carrying" is a good term for the means of spreading ♀ s of *Diurnea fagella* and *Hybernia marginaria*; as we know them these might want much carrying.

Having given us the opinion that these particular melanic races are the result of "mutations," we come to the general statement, that Mr. Mansbridge "broadly classes all instances of melanochroism, and leucochroism as Darwinian variations," and "all cases of melanism and albinism as well as yellow to red, or red to yellow, and similar changes where the break is sudden, as mutations or De Vriesian variations."

Leaving De Vries out of the question, we wonder what this means. Are all the slightly shaded, much shaded, completely fuscous, and black specimens of *Hybernia marginaria*, melanic, and all equally the result of leaps, just as far as the individual specimen has got, or are the intermediate ones produced "as Darwinian variations" (!) whilst the uttermost ones are produced by "mutations"? If this is the idea, we wonder how it is done.

Leaving this question of melanism, Mr. Mansbridge has, in his further statement, given much food for thought. He considers "all cases of change from yellow to red, or red to yellow, and similar changes where the break is sudden as mutations," *i.e.*, that those variations that are most effective in their appeal to the human eye are "mutations," and those that are less so are "Darwinian variations," and herein the nakedness and poverty of argument, and want of appreciation of the facts lie bare. Is it at all necessary to assume that the difference between a yellow hindwing and a red hindwing, in *Arctia caia*, is greater, biologically, than between an ordinary typical *Acidalia virgularia*, and its *suffused* London form, or that a yellow-spotted aberration of *Anthrocera trifolii* has a greater biological significance than say the difference in the colour of ♂ *Spilosoma mendica*, compared with that of the ♀? Do the eggs from a yellow-spotted ♀ of *Anthrocera trifolii*, or of a yellow hindwinged *Arctia caia*, or a suffused *Abraxas sylvata* (*ulmata*) of necessity produce progeny with "yellow spots," "yellow hindwings" or "suffused" wings respectively? Do they produce necessarily their own "leap"-kind to perpetuate the marvellous biological wonder? We know well they do not.

We would like, without impertinence, to again refer Mr. Mansbridge, not only to our remarks (*Brit. Noctuae*, etc., ii., pp. ix *et seq.*) and the suggestions that arise from a study of the advance of ochreous and yellow forms to red or brown, in *Colias edusa*, *Gonepteryx cleopatra*, *Rumia crataegata*, *Arctia caia*, *Nemcophila plantaginis*, *A. villica*, *Coenonympha pamphilus*, *C. davus*, *Epinephele tithonus*, *Callimorpha hera*, *Satyrus semele*, and many other species, but we would like to call his attention to the retrogressive condition arising, possibly, in an entirely different direction in *Callimorpha dominula*, *Anthrocera filipendulae*, *A. trifolii*, *A. lonicerae*, *A. purpuralis*, *A. achilleae*, *Catocala nupta*, *Cosmotriche potatoria*, *Pachygastria trifolii*, *Eutricha quercifolia*, and a whole host of Noctuids and Geometrids. We would ask for a fair criticism of the facts known and theories advanced; these haphazard statements which mean nothing, and lead nowhere, are getting wearisome.

Our older and well-informed lepidopterists will doubtless say, why treat so small a matter so seriously. My answer is that, if only our older and well-informed lepidopterists were readers of the entomological magazines, it would indeed be unnecessary, but year by year new and young members come in, who, of necessity, are ignorant of the work that has been done, and yet are anxious to learn. There is a time when oft-repeated errors, erroneous opinions and ill-judged statements, sink as facts into the minds of those who know no better, and one learns very early in the educational field, that it is more difficult to eradicate an error, than it would have been at first to teach the truth. Hence it becomes necessary, now and again, to ask those who, in their turn, would profess to teach, to themselves make sure of their ground, to remember that assertion does not necessarily include accuracy either in fact or argument, that what the best know is but a trifle, and that to teach others even a part of that trifle, one must make sure of one's own facts first. It appears to be unfortunate that the few reprints that were at the time made of the essays on "The genetic sequence in Insect Colours," and "Secondary Sexual Characters in Lepidoptera," and which appeared as introductory chapters to vols. ii and iii of *The British Noctuae and their Varieties*, were so soon dispersed.

They unfortunately appear to be largely overlooked owing to not being prominent in their present positions; still the facts are in the hands of most lepidopterists, and they can still be got at by those who will.

The "British List."

By W. E. SHARP, F.E.S.

Mr. Day, in his interesting record of the reappearance in Carlisle of the long lost *Brontes planatus* (not "Brantes phanatus") (*Ent. Record*, xx., 62) remarks—"whether casual immigrants of this kind should be considered as British insects is doubtful, unless they breed and establish themselves, when there can be little reason for not admitting them to our list."

Such a record and the comment thereon, perhaps not unnaturally, suggest reflections as to the true meaning and inwardness of this "British List" whose validity and content appear to many of us a matter of considerable interest and importance, and perhaps I may be pardoned if I venture to discuss a little more at length the merits of the case, and the definition of the term a little more fully.

Now, it would seem that there are two quite distinct senses in which the "British," or indeed any list of a circumscribed faunistic area, may be used, two ideas which the same term connotes, hence the possibility of difficulty or confusion in its application.

One constantly hears discussed the claim of some doubtful native to a place in this exclusive "List;" indeed the entomological public is sometimes tacitly invited to constitute itself into a kind of court of appeal on the right of entry of such "destitute aliens" of the *Insecta*. One has heard suggestions of a "time limit," a fixed number of years, after which, should the applicant have proved himself capable of an honest and respectable livelihood in these islands, letters of naturalisation should be granted.

Now, I venture to think that, in a true or really scientific sense, no such naturalisation is possible, admission must depend on a lineage vastly more ancient; for I would define as "British" in the faunistic sense, only that assemblage of insects which had become established here by exclusively natural means, those in which man, with all his activities, his ships, and his commerce, had no part. The test would be original natural establishment, and, by establishment, I would mean survival over such a space of time as would include all possible climatic vicissitude. In this sense I should regard the claim of such a species as *Rhynchites bacchus*, even if it could be demonstrably proved to have been extinct here for fifty years, much more admissible than that of the too familiar *Blattae* of our kitchens, or of many of the ubiquitous *Nipti*, *Bruchi*, and *Cryptophagi* of our granaries. Most certainly would I repudiate the idea that the "British List" should be in any degree a kind of census of the entomological population of the kingdom on the date of its publication. On the contrary I hold the "British List" of science to be practically a closed book, closed with a few possible exceptions before the historic period, closed long before the first Phœnician mariner sighted the unknown Cassiterides.

That such an original establishment of the British fauna took place, from whatever quarter the immigration may have come, during the period which elapsed between the end of the glacial age and the